Ten years ago Maris Vinovskis published an influential essay that signaled a profound shift in Civil War scholarship. Although thousands of books and articles had been written about the military experiences of Civil War participants, Vinovskis contended that relatively little was known about their actual lives. Vinovskis called explicit attention to the demographic cost of the war and its continuing influence on the life course of the Civil War generation. An estimated 618,222 military deaths occurred during the war—roughly equal to the number of deaths suffered in all other American wars through the Korean War combined. The human cost of the Civil War becomes even more spectacular when one considers the rate of death. Nearly one in eight white men of military age died during the war, exceeding the rate of death in World War II by a factor of six, and the rate of death in the Vietnam War by a factor of 65.

During the last decade historians have answered Vinovskis’s call for a social history of the Civil War. We now have several excellent monographs on women’s response to the war, numerous studies of the northern and southern “home fronts,” dozens of community studies that illuminate how race, class, and gender shaped the wartime experiences of ordinary Americans, and a growing number of studies on the wartime and Reconstruction experiences of African Americans. Few studies, however, have looked beyond the immediate stress of the war to examine its long-term consequences, and virtually no research has built upon Vinovskis’s preliminary demographic speculations to examine the war’s impact on postwar population. The lack of long-term study is most regrettable in the South, which lost an estimated one in four white men of military age—three times the rate of death in the North.

In this dissertation I rely on new national samples of the 1850, 1860, 1870, 1880, 1900, and 1910 United States censuses to investigate short- and long-term trends in the white population and, where possible, to estimate the war’s impact on marriage and fertility. These samples—part of the Integrated Public Use Microdata Series (IPUMS)—include data on more than 1.65 million individuals living in 357,000 households. The dissertation also relies on a preliminary longitudinal sample of Union Army recruits currently being constructed at the University of Chicago as well as more conventional qualitative sources.

Despite their seeming precision, estimates of the number of military deaths in the Civil War are crude approximations. Estimates of the number of noncombat deaths that occurred in the Confederate forces, whose medical records did not survive the war, are based on the crude assumption that disease had an equal impact on Confederate and Union troops. There are a number of reasons to question this assumption, including the growing shortage of food, clothing, and medicine in the Confederate Army in the last few years of the war, and the lack of voluntary organizations in the South comparable to the U.S. Sanitary Commission and Western Sanitary, which helped improve the sanitary conditions of Union Army camps. Perhaps most importantly, the South’s relatively lower urbanization and population
density increased the probability that a typical Confederate soldier entered the war without acquired immunities to infectious disease. Consequently, Confederate soldiers may have suffered disproportionately from disease mortality during the war.

Chapter 2 revisits estimates of Civil War disease mortality through a case study of its leading killers: diarrhea and dysentery. Kaplan-Meier survival analysis of data from the Union Army sample reveals that prior exposure to disease (as measured by the proxy of farm/nonfarm occupation before enlistment) proved to be a significant determinant of mortality from nonspecific diarrhea. Recruits with nonfarm occupations before the war enjoyed a 60 percent lower risk of death from "diarrhea" and "dysentery" than recruits with farm occupations over the course of a typical 36-month enlistment. Using these data and anecdotal evidence from other studies, the chapter suggests that estimates of disease mortality from nonspecific diarrhea in the Confederate Army are approximately 10 to 20 percent too low.

Chapter 3 relies on the IPUMS census samples to examine the war's impact on trends and differentials in the timing and incidence of white marriage. It focuses on the potential existence of a "marriage squeeze" on a generation of white women after the war. Although social historians have hypothesized that the relative lack of marriageable men in the postbellum era led to a more competitive marriage market for women, especially in the South, the impact of the war on marriage has yet to be quantified. The nineteenth-century census samples pose some significant methodological problems for the study of nuptiality, however. Although the 1880 census was the first census to record individuals' marital status, it did not record the duration of their current marital status or the number of times they had been married. As a result, only the timing and incidence of individuals' first marriage at the time of the census can be estimated; short-term trends in first marriage and short- and long-term trends in remarriage are impossible to discern. More critically, the 1850-1870 censuses did not record individuals' marital status. Fortunately, the censuses contain enough information—surname, sex, age, and position in household—to reliably infer marital status.

Estimates of the median age at first marriage by census year, census division, and sex for native-born whites suggest that the war's impact on marriage was moderate. Overall, the median age at first marriage for both men and women was somewhat lower in 1850 than in 1880, confirming indirect evidence of declining nuptiality in the nineteenth century. In the short term, the results for the southern census divisions seem to confirm the expected impact of the war: the median age at first marriage for southern white males fell 0.4 years between 1860 and 1870 while the median age for southern white females rose 0.5 years. Over the same period, there was little overall discernible change in the marriage age in the North. From a longer-term perspective, however, the evidence of a war-related marriage squeeze on southern white women is more ambiguous. The median age at first marriage for both southern white males and southern white females in 1850 is almost identical to the median age at marriage in 1870. In addition, large east/west differentials in the timing and incidence of marriage, the continuing increase in the median age at first marriage after 1870, and the long-term decline in the proportion of the population that eventually married

6 Disease-mortality differentials between the two armies from acute infectious diseases such as smallpox and measles may have been even higher. Chulhee Lee found large differentials in acute infectious disease in his study of Ohio recruits in an earlier version of the Union Army recruit dataset. Lee, "Socioeconomic Background."

7 I relied on imputed relationships included in the 1850–1870 public use samples to construct an "ever-married" variable. Men and women with imputed spouses or children present in the household were considered "ever-married" and those without apparent spouses or children present were considered "never-married." When applied to the 1880 census the imputation proved accurate 97 percent of the time. Although the inferred ever-married variable does not distinguish between married, divorced, widowed, or separated men and women—all are considered "ever married"—tabulating the proportion ever-married by single years of age allows the timing and incidence of first marriage to be calculated.
Summaries of Dissertations

suggest that economic factors, such as the differentials and trends in the price of farmland, played a more significant role in determining nuptiality than demographic factors.8

Chapter 4 indicates that the war had a dramatic, if short-term, impact on fertility. I construct new estimates of the white birth rate in the United States using back-projection techniques outlined by Ansley Coale and Melvin Zelnik and new estimates of nineteenth-century fertility recently published by Michael Haines.9 Sectional estimates of white births indicate that the war resulted in a deficit of approximately 1.2 million white births between 1861 and 1870—approximately twice the number of men killed in the conflict. Although the fertility deficit was smaller in absolute numbers in the South than in the North, the percentage of the expected number of births was much larger in the South. During the years 1862–1866 the South suffered an average annual fertility deficit of 21.0 percent, almost twice the North’s 10.9 percent deficit. The difference probably reflects the higher proportion of southern men who participated in the conflict than northern men, and the greater economic stresses and uncertainties in the Confederate South.

Chapter 4 also estimates underenumeration errors in the 1850, 1860, 1870, and 1880 censuses. These results indicate that net census underenumeration ranged between 6 and 9 percent in the four censuses, although some groups, such as infants and women over age 30, were far more likely to be undercounted than other groups. Regional estimates of underenumeration confirm the long-held belief that the 1870 census undercounted parts of the U.S. South relative to other regions. The level of underenumeration, however, is far less than contemporaries believed. Indeed, the new estimates of underenumeration in the 1870 South suggests that previous adjustments used to “correct” for the perceived level of southern undercounts resulted in a larger error than the original count.

Chapters 5 and 6 shift the focus of the dissertation from the short-term impact of the Civil War on the white population to the study of long-term trends and determinants of fertility. Using own-child methods of fertility estimation, Chapter 5 estimates trends and differentials in total fertility by census division and various population subgroups. Marital fertility rates and various indices of marital fertility are also constructed to investigate the extent of conscious family limitation by census division and year. Despite the long-term decline in U.S. birth rates after 1800, I find no clear evidence that white couples in 1850 were able to effectively curtail childbearing after reaching a desired number of children. By 1860, however, couples in the New England and the Mid-Atlantic census divisions were practicing effective “stopping” behavior, and by 1870, couples in the East-North Central division were consciously limiting their pregnancies as well. There is no clear evidence that southern white couples between 1850 and 1880 practiced effective parity-dependent fertility control.

Finally, Chapter 6 investigates correlates of marital fertility among native-born women of native-parentage, the first group to effectively limit their births. Most researchers have stressed the economic determinants of fertility in the nineteenth century. Without denying the importance of economic factors, the chapter focuses on possible cultural determinants of fertility, specifically the relationship between religion and birth rates within marriage. Although no direct assessment is available of parents’ religious affiliation or religiosity, indirect measures prove to be significantly correlated with the marital fertility. The proportion of own children with biblical names—believed to be a rough proxy of parental religiosity—is positively associated with marital fertility. County-level census data of church seating capacities also indicate that the presence of Congregationalists and Universalists is associated with lower marital fertility whereas the presence of Lutherans is associated with

8 Although the war appeared to have only a modest impact on the timing and incidence of first marriage, it may have had a more dramatic impact on remarriage. The much higher rates of widowhood in the South evident in the 1880 IPUMS sample supports this conclusion, although it is impossible to make a conclusive test of this hypothesis without data on the number of times each individual had been married and the duration of their current marital status.

9 Coale and Zelnik, New Estimates; and Haines, “Estimated Life Tables.”
higher marital fertility. These results are consistent with the hypothesis that “liberal” religious beliefs reduced cultural impediments to adopting family limitation strategies. The fact that early advocates of birth control in antebellum America were religious liberals or free-thinkers further supports this conclusion.

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The development of real income series that are comparable both over time and across countries significantly expanded the possibilities for research in quantitative economic history and long-run economic growth. This dissertation re-examines the long-run GDP estimates that span over 100 years (henceforth, long-span GDP estimates). While these estimates are used extensively in the literature, their reliability has received limited attention. My findings are first, that there are persistent discrepancies between the long-span GDP estimates and alternative benchmark estimates of comparative GDP. Second, the long-span GDP estimates may result in the mismeasurement of economic performance for individual countries and the forces of catch-up and convergence across countries.

Two problems arise in the calculation of long-span GDP estimates. First, given that exchange rates do not accurately reflect relative purchasing power, GDP-level comparisons are possible only with the appropriate purchasing power parity converters. This requires

This dissertation was completed in 2000 in the Department of Economics at the University of Miami (FL) under the supervision of Professor John Devereux. Professor Luis Locay also contributed significantly to this work’s progress.

For example, these data are central to the large body of work on catch-up and convergence. The seminal papers in this area are Abramovitz, “Catching Up”; and Baumol, “Productivity Growth.” Elsewhere in the growth literature they have been used to test competing growth theories in papers such as Kockerlakota and Yi, “Is There Endogenous Long Run Growth.”

The purchasing power parities (PPP) that are referred to are not those related to equilibrium exchange rates. Instead they have been developed only to allow price and volume comparisons of GDP. They refer to the entire range of goods that make up GDP, including goods that are not traded.
Summaries of Dissertations

REFERENCES


**Discussion of Hacker, Ward, and White**

My assigned task is to discuss the three fine dissertations chosen as finalists for the Allan Nevins Prize. But, I want to start by mentioning the eight other dissertations that I read as part of the competition. I found it truly inspiring that so much good work is being done in American economic history. All 11 dissertations submitted were truly first rate and all of them deserved a chance to be recognized. So, to all who submitted, I want to say congratulations on a job well done. And, to the three who were chosen as finalists, I want you to realize just how special your dissertations really are. There were not only the best; they were the best of a very impressive group.

The other thing that I want to mention is taste. I tried very hard to be broad and eclectic in my preferences. But, I discovered as I was reading the submissions and choosing finalists that I am in many ways a prisoner of my training. I received my Ph.D. in economics in 1985, at a time when the “new economic history” really was not new any more. But, perhaps because Peter Temin gave me my first introduction to the field, some of the tenets of that revolution nevertheless found their way into my soul.

One thing that I learned is the importance of formulating a clear hypothesis and marshaling evidence to support or refute it. I find that I react very poorly to story-telling. I am constantly wondering, “What is the question being asked? What is the evidence?” I find it quite easy to accept a wide variety of kinds of evidence. Big data sets are lovely, but so are scattered numbers painstakingly gleaned from newspapers or archival records. And sometimes quotations from company or government records provide the clearest proof of what was going on. But I really want to see evidence logically assembled to answer a well-posed question.

Many of the dissertations submitted had this crucial characteristic. What distinguished the three finalists were the importance of the questions they ask and the quality of their evidence. All three address crucial issues in American economic history and change the way that we view the past. I will discuss them in alphabetical order.

James Hacker has written a monumental study in American demographic history. He was a participant in the Minnesota Historical Census Projects that created computerized samples of the U.S. Censuses of Population for the second half of the nineteenth century. His dissertation contains no fewer than five distinct studies of the demographic changes experienced by the white population between 1850 and 1880. He is particularly interested in the effects of the Civil War on the demography of the American South.

The Minnesota center is an example of a research framework we do not often see in economics. It involves many people doing cooperative research—each one tackles a small piece and the whole is greater than the sum of the pieces. Hacker’s dissertation is a microcosm of the Census Project. No one of Hacker’s studies makes you bang your fist on your
head and exclaim, “I wish I had thought of that!” But, taken together the dissertation is an extraordinary achievement. It furthers our understanding of nineteenth-century American demographic history greatly and challenges many long-held beliefs.

Hacker’s first study actually uses data from another of these cooperative data ventures—Robert Fogel’s project on Union Army recruits. In this study Hacker attempts to estimate the number of Confederate soldiers who died of diarrhea and dysentery. While this is certainly not a glamorous topic, Hacker is thoroughly convincing that it is interesting and important. Indeed, I have to give Hacker credit for opening my eyes to the prevalence of disease-related deaths among soldiers on both sides. Ever since I read a biography of Florence Nightingale in fifth grade, I have known that camp conditions and health care were abysmal. But, until I read Hacker’s dissertation I certainly never realized that the majority of deaths during the Civil War were due to disease, not to battle wounds.

Thanks to spectacular Union Army medical records, we have excellent data on the cause of death of Northern soldiers. These records show that roughly one-quarter of the Union soldiers who died of disease died of diarrhea and dysentery. Scholars have estimated the number of Southern deaths from these causes, for which the records burned in the Richmond fire of 1865, by applying the same proportion to Southern enlistments. What Hacker is able to do using the new data on Union Army recruits is to see what death from diarrhea and dysentery are correlated with. He finds that Union recruits from farms were substantially more likely to die from these enteric diseases than urban recruits. Since a larger fraction of Southern soldiers were farmers, it follows that more Confederate soldiers probably died of diarrhea and dysentery than previously believed.

Does this finding fundamentally change our world view? Surely not. But, it does give us a more accurate picture of both the human suffering of the Civil War and the disadvantages faced by the South. And, it is indeed ironic that many Southerners believed that Southern soldiers would be superior because they were healthy farm lads, not puny Northern factory workers. We discover from Hacker’s dissertation that fetid urban factory conditions, by exposing future soldiers to disease, yielded some immunity to the diseases prevalent in Civil War camps.

Two of Hacker’s studies look at the demographic consequences of the Civil War on the South. One asks whether the decimation of the male population led to a “marriage squeeze” for women; that is, did it make it hard to find a husband in the South after the war? The other asks what effect the war had on births in the South during and after the war. In both cases Hacker comes up with surprising conclusions. He finds that the horrendous rate of Southern war casualties actually had little effect on the age of South em women at marriage or the likelihood that Southern women would never marry. The war did, however, have a large impact on births. Hacker finds that the shortfall of Southern births from their trend level (as a fraction of the expected number of births) was almost double the percentage shortfall in the North and larger than previously estimated.

Both of these studies are largely descriptive. That is, there is an air of Joe Friday about them—“just the facts, ma’am.” Hacker often has tantalizing speculations about why various things occurred. For example, he hypothesizes that the female marriage age did not change after the war because age differentials between men and women may have grown or the remarriage rate for men may have risen. I have to confess that I would have liked to see him write fewer papers and pursue some of these hypotheses more seriously.

A by-product of Hacker’s analysis of birthrates is that he is able to use survival analysis to estimate the degree of census under-enumeration in various years. For a century now scholars and census officials have believed that the 1870 census greatly underestimated the Southern population and so a correction factor of nearly 10 percent has been added. The microdata Hacker uses suggests that there was some under-enumeration in the South in 1870, but it was not nearly as large as previously thought. Indeed, he finds that the truth is probably closer to the original published count than to the adjusted version. This by-product
of Hacker’s study is probably more significant than his finding about births. For example, one can only wonder what impact these new population estimates may have on our estimates of per capita Southern output. They could have a fundamental effect on the whole debate about postbellum Southern stagnation and decline.

The last two chapters of Hacker’s dissertation deal with the decline in fertility in the nineteenth-century United States. A well-known fact is that American fertility declined steadily from 1800 to 1900. This finding is confirmed by Hacker’s analysis of microdata. But Hacker is able to plumb this change more deeply. Using own-child estimates of fertility, he finds little evidence of stopping behavior (parity-dependent birth control) before 1850. This finding raises more questions for me than it answers. For example, if it was not stopping behavior, what allowed the decline in fertility before 1850? What changed in 1850? Hacker has some tantalizing speculations that I would love to see him pursue.

In many ways Hacker saves his best work until last. The final chapter analyzes the role of culture in determining marital fertility. Economic historians often attribute the decline in marital fertility to economic determinants such as land availability and income. But, it is certainly possible that cultural factors, such as religion, may have been important. Hacker implements a clever test of whether religiosity might be key. He uses the proportion of children’s names in a family that are biblical as a proxy for religiosity. In essence, he tests whether couples that name their children after biblical figures have higher fertility than those who do not. Hacker is well aware that his independent variable is imperfect. But he is very persuasive that people in the mid-nineteenth century were actively choosing names rather than following tradition. He marshals a plethora of primary sources to show that the use of biblical names did reflect the religious convictions of the parents.

Hacker finds that religiosity had a large impact on marital fertility. In regressions with controls for region, literacy, husband’s occupation, and many other factors commonly thought to be correlated with fertility, the prevalence of biblical names had a positive, statistically significant, and quantitatively important impact on fertility. This finding surely moves our priors toward thinking that social and cultural changes, such as declining religiosity, may have been important forces behind the demographic transition of the nineteenth century.

Let me turn next to Marianne Ward’s dissertation. Comparative estimates of the behavior of real output per capita over long periods play a central role in shaping our views of the process of economic growth. They provide critical evidence concerning whether followers tend to catch up to leaders; whether there is overall convergence in real incomes; what countries were the world leaders in real income in different eras; and when the United States became the world’s leading economy and how its growth performance compares with that of other countries.

The motivation for Ward’s dissertation is the observation that almost all comparative long-term estimates of real output rely on the same methodology. As Ward ably explains, this methodology involves two steps. The first is to obtain comparative estimates of real output per capita for a base year. This is done by taking standard estimates of GDP per capita in domestic currency units and then adjusting them for differences in the currencies’ purchasing power. The base year is typically fairly recent. The second step is to project the estimates of per capita real output for each country backward using conventional estimates of the country’s real growth; these estimates are typically those produced by the country’s own statistical agency. This is the methodology underlying Angus Maddison’s widely used comparative estimates.

Ward points out that this methodology has three major drawbacks. First, small errors in estimating growth rates, or small conceptual differences in the measurement of real output, will compound over long periods to produce large differences in the estimated levels of output per capita. Second, the base-year estimates are calculated using prevailing international prices in the base year, while the backward projections are performed using each
country's prices. As a result, the estimates of real output per capita for different countries prior to the base year are not comparable: different weights are implicitly being used in different countries. And third, in cases where the domestic growth rates are not based on chain-weighting, the estimates implicitly put considerable weight on products, notably services, whose relative prices have risen over time.

Motivated by this observation, Ward sets out to construct more useful comparative long-term estimates of real output per capita. Her dissertation is a perfect illustration of the adage that genius is one part inspiration and 99 parts perspiration. The inspiration is the realization that comparative long-term estimates constructed using benchmark comparisons in multiple years are likely to be much more reliable and useful than estimates based on the traditional methodology—especially if some of the benchmark years are early in the sample. The perspiration begins with scouring the historical and modern literatures and the archives for data that can be used to construct estimates of currencies' relative purchasing power at different times. The range of sources Ward's hard work has turned up is remarkable: from the 1874 report of the Massachusetts Bureau of Statistics of Labor, to the 1914 South African Economic Commission Report, to work by the Ford Motor Company in 1930, to the modern International Comparison Project. She then carefully and painstakingly uses the raw data to construct estimates of overall purchasing power, and combines them with nominal GDP estimates, to create new comparative estimates of real GDP per capita at different times.

This detailed examination of the nitty-gritty of the data turns out to be crucially important. Even for a year as recent as 1950, Ward's approach yields estimates for several countries of real GDP per capita relative to the United States that differ by more than 20 percent from the standard estimates. When we go back to the beginning of the sample in 1872, in several cases the differences are more than 50 percent. And in the case of Italy, Ward's procedure leads to a revision in our estimate of its GDP per capita in 1872 relative to the United States of an astonishing factor of three. As Ward shows, these revisions significantly change the evidence about a wide range of important issues, including when the United States overtook the United Kingdom as the world leader in real income per capita and the overall performance of the United States relative to the major European economies.

Ward's analysis is so persuasive and her results are so important that I find it hard to understand why no one followed her approach sooner. But, I can only say that I am glad Ward finally did. I have no doubt that her estimates will become a crucial input to future studies. She has given economic historians a great gift—better data on a crucial topic.

Nevertheless, I would like to close with a complaint that is easy to make: I wish that Ward had done more. I mean this in two respects, one minor and one not-so-minor. The minor respect is that I wish she had gone further in investigating the implications of her new estimates. For example, her analysis of catch-up does not go much beyond a few summary statistics. It would not be terribly hard, and it would be very instructive, to run some simple statistical tests. The places to start would be a basic Baumol-style convergence regression and a slightly more complicated De Long-style procedure that accounts for the fact that even the improved GDP estimates still suffer from some error. I would have liked to know what results these procedures produced when applied to the new data, and how the results differed from those that are obtained from the standard estimates.

The not-so-minor respect is that I feel that Ward has stopped just short of putting the last nail in the coffin of the traditional methodology. Her approach is logically compelling; she marshals a convincing array of evidence that the price indexes underlying her estimates are reliable; and she shows persuasively that auxiliary evidence about other countries' economic performance relative to the United States is much more consistent with her estimates than with the traditional ones. But the nail that is missing is an explanation of how the traditional approach can have gone so far awry. An error of 20 percent in the estimate of a country's income relative to the United States 40 years before the base year requires a
The second half of White's dissertation deals with quality changes and the slow diffusion of the tractor. White does a masterful job of deriving a quality-adjusted price index for tractors for 1918–1955 using hedonic price regressions. Whereas the real price of tractors unadjusted for quality changes was roughly constant for the first 20 years after World War I, White finds that the quality-adjusted price plummeted. In this derivation he has again marshaled a marvelous variety of unusual sources, such as collectors' guides to antique tractors and the annual results of the University of Nebraska’s tractor trials.

White then tests whether this falling price can rescue threshold models of the diffusion of the tractor. A recurrent finding in the literature is that threshold models suggest that tractors should have diffused much more quickly than they did. White finds that taking into account the falling quality-adjusted real price gives a slower decline in the threshold size. Thus, he argues his hedonic adjustment can reconcile actual diffusion and the threshold model.

I have to confess that I find this chapter of White's dissertation the least persuasive. The truth is all threshold models seem to me to put too much emphasis on farm size. Farm size is clearly an important variable—but other things surely matter. Some obvious examples are soil type and texture, the ability to share farm implements with neighbors, and the farmer’s bodily strength and felicity with machines or animals. It seems to me that White limits the usefulness of his contribution here by tying it to a threshold model. Almost any model of diffusion will find diffusion rising when the real quality-adjusted cost of the technology falls. By showing us the dramatic change in the effective price of tractors, White builds a prima facie case that it is no surprise that diffusion was gradual rather than immediate.

The other thing that struck me was the fundamental similarity between White’s finding and Olmstead’s finding about the gradual diffusion of the reaper. White emphasizes how his results go against Olmstead’s critique of threshold models of the reaper. But the thing I remember most from Olmstead’s work was the tremendous changes he documents in the quality and usefulness of reapers. Well, that is much of what White finds about tractors—they improved greatly over their first couple of decades of availability. This fact, not the somewhat limited and artificial threshold calculation, is the crucial insight of White’s analysis of diffusion.

Although I have tried hard to think of at least one piece of constructive advice for each of the finalists, I hope that my admiration for all three dissertations is obvious. Based on these fine dissertations and the other excellent submissions, I can honestly say that the field of American economic history is not merely alive and well, it is truly flourishing.

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